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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-On-Hudson, N. Y.

ADDRESS TO THE BOTANICAL SECTION OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE[1]

To preside over the Botanical Section on the occasion of its first meeting in Australia is no slight honor, though it also imposes no small responsibility. We members from Great Britain have a deep sense of the advantage which we derive from visiting these distant shores. I am doubtful whether any scientific profit we can confer by our coming here can balance that which we receive; while over and above this is the personal kindliness of the Australian welcome, which on behalf of the visitors of this section from the old country I take this opportunity of gratefully acknowledging. Of the members of the British Association, those who pursue the national sciences may expect to gain most by their experiences here; and perhaps it is the botanists who stand to come off best of all. Living as most of us do in a country of old cultivation, the vegetation of which has been controlled, transformed, and from the natural floristic point of view almost ruined by the hand of man, it is with delight and expectation that we visit a land not yet spoiled. To those who study ecology, that branch of the science which regards vegetation collectively as the natural resultant of its external circumstances, the antithesis will come home with special strength, and the opportunity now before them of seeing nature in her pristine state will not, I am sure, be thrown away.

I may be allowed here to express to the Australian members of the Section my regret that the presidency for this occasion

[1] Australia, 1914.

should not have fallen to one who could with unusual weight and knowledge have addressed them from the floristic and geographical point of view. I mean, to Professor Bayley Balfour, of Edinburgh, who was actually invited by the council to preside. He could have handled the subject of your rich and peculiar flora with detailed knowledge; and, with the true Hookerian touch, he would have pictured to you in bold outlines its relation to present problems. Failing such equipment, I may at least claim to have made some of your rare and peculiar forms the subject of special study at intervals spread over thirty years: for it was in 1884 that I was supplied with living plants of *Phylloglossum* by Baron Ferdinand von Müller, while a paper to be published this year contains details of a number of ferns kindly sent to me by various collectors from New Zealand. I have been personally interested more especially in your rare Pteridophytes, isolated survivals as they surely are of very ancient vegetation. I propose to indicate later in this address some points of interest which they present. But first I shall offer some more general remarks on the history of the investigation of the Australian flora, as a reminder of the recent death of Sir Joseph Hooker, whose work helped so greatly to promote a philosophical knowledge of the flora of this quarter of the globe.

Few, if any, of the large areas of the earth's surface have developed their coat of vegetation under such interesting conditions as that which bears the Australasian flora. In its comparative isolation, and in its freedom from the disturbing influence of man, it may be held as unique. We may picture to ourselves the field as having been open to evolutionary tendencies, unusually free from the incursion of competitive foreign types, and with its flora shaped and determined through long ages in the main by climatic influences. Naturally the controlling effect of animal life had been present throughout, as well as that of parasitic and fungal attack; but that potent artificial influence, the hand of man, was less effective here than in almost any other area. The aborigines were not tillers of the soil: in their digging for roots and such-like actions they might rank with the herbivorous animals, so far as they affected the vegetation. Probably the most powerful influence they exercised was through fire. And so the conditions remained, the native flora being practically untouched, till the visit of Captain Cook in 1770: for little account need be taken of the handful of specimens collected by Dampier in the seventeenth century.

Captain Cook shipped with him in the *Endeavour* a very remarkable man, viz., Joseph Banks, whom Dr. Maiden has described as "the Father of Australia." He not only acted as the scientific director of the expedition, but he was also its financier. Educated at Eton and Oxford, he found himself as a young man possessed of an ample fortune. Though devoted to field sports, he did not, like so many others, spend his life upon them. Following the dictates of a taste early awakened in him, he turned his attention to travel for scientific ends. His opportunity came when Cook was fitting out the *Endeavour* for his first voyage to the southern seas. Banks asked leave of the Admiralty to join the expedition, which was granted, and he furnished all the scientific stores and a staff of nine persons at his own expense.

The story of that great expedition of 1768 to 1771 is given in "Cook's Voyages," compiled by Dr. Hawkesworth, a book that may be found in every library. Though it is evident throughout that Banks took a leading part in the observational

work of the expedition, it has not been generally known how deeply indebted Hawkesworth was to Banks for the scientific content of his story. This became apparent only on the publication of Banks's own journal 125 years after the completion of the voyage. The circumstances of this have a local interest, so I may be excused for briefly relating them.

Banks's papers, including the MS. journal, passed with his library and herbarium on his death to his librarian, Robert Brown. On the death of the latter they remained in the British Museum. But after lying there for a long period they were claimed and removed by a member of Banks's family, and were put up for auction. The journal was sold for £7 2s. 6d., and the last that has been heard of it is that it came into the possession of a gentleman in Sydney. Perhaps it may be lying within a short distance of the spot where we are now met. This valuable record, fit to rank with Darwin's "Voyage of the *Beagle*," or Moseley's account of the "Voyage of the *Challenger*," might thus have been wholly lost to the public had it not been for the care of Dawson-Turner, who had the original transcribed by his daughters, helped by his grandson, Joseph Dalton Hooker. The boy was fascinated by it, and doubtless it helped to stimulate to like enterprises that botanist to whom Australia owes so much. The copy thus made remained in the British Museum. Finally, from it in 1896 Sir Joseph Hooker himself edited the journal, in a slightly abridged form. It is now apparent how very large a share Banks actually took in the observation and recording, and how deeply indebted to him was the compiler of the account of the voyage published more than a century earlier, not only for facts, but even for lengthy excerpts.

The plants collected in Australia by this expedition amounted to some 1,000 species, and with Banks's herbarium they found, after his death, a home in the British Museum. Several minor collections were subsequently made in Australia, but the next expedition of prime importance was that of Flinders in 1801 to 1805. What made it botanically notable was the presence of Robert Brown. Hooker speaks of this voyage as being, "as far as botany is concerned, the most important in its results ever taken." The collections came from areas so widely apart as King George's Sound, southern Tasmania, and the Gulf of Carpentaria. These, together with Banks's plants and other minor collections, formed the foundation for Brown's "Prodromus Floræ Novæ Hollandæ," a work which was described in 1860 by Sir Joseph Hooker as being "though a fragment . . . the greatest botanical work that has ever appeared." It was published in 1810. I must pass over without detailed remark the notable pioneer work of Allan Cunningham, and of some others. The next outstanding fact in the history of Australian botany was the voyage of Ross, with the *Erebus* and the *Terror*: for with him was Joseph Hooker, whose botanical work gave an added distinction to an otherwise remarkable expedition.

The prime object of the voyage was a magnetic survey, and this determined its course. But in the intervals of sailing the Antarctic seas the two ships visited Ascension Island, St. Helena, the Cape, New Zealand, Australia, Tasmania, Kerguelen Island, Tierra del Fuego, and the Falkland Islands. Thus Hooker had the opportunity of collecting and observing upon all the great circumpolar areas of the southern hemisphere. He welded together the results into his great work "The Antarctic Flora." It was published in six large quarto volumes. In them about 3,000

species are described, while on 530 plates 1,095 species are depicted, usually with detailed analytical drawings. But these magnificent volumes did not merely contain reports of explorations, or descriptions of the many new species collected. There was much more than this in them. All the known facts that could be gathered were incorporated, so that they became systematically elaborated and complete floras of the several countries. Moreover, in the last of them, the "Flora Tasmaniæ," there is an introductory essay, in which the Australasian flora was for the first time treated as a whole, and its probable origin and its relation to other floras discussed. Further, questions of the mutability and origin of species were also raised in it. The air was full of such questions in 1859; the essay was completed in November of that year, less than twelve months after the joint communications of Darwin and Wallace had been made to the Linnean Society, and before the "Origin of Species" was published. It was to this essay that Darwin referred when he wrote that "Hooker has come round, and will publish his belief soon." But this publication of his belief in the mutability of species was not merely an echo of assent to Darwin's own opinion. It was a reasoned statement, advanced upon the basis of his "own self-thought," and his own wide systematic and geographical experience. From these sources he drew support for "the hypothesis that species are derivative and mutable." He points out how the natural history of Australia seemed especially suited to test such a theory, on account of the comparative uniformity of the physical features being accompanied by a great variety in its flora, and the peculiarity of both its fauna and flora, as compared with other countries. After the test had been made on the basis of the study of some 8,000 species of plants, their characters, their spread, and their relations to those of other lands, Hooker concluded decisively in favor of mutability, and a doctrine of progression. After reading this essay, Darwin wrote that it was to his judgment "by far the grandest and most interesting essay on subjects of the nature discussed I have ever read."

But beyond its historical interest in relation to the "Origin of Species," Hooker's essay contained what was up to its time the most scientific treatment of a large area from the point of view of the plant-geographer. He found that the Antarctic, like the Arctic flora, is very uniform round the globe. The same species in many cases occur on every island, though thousands of miles of ocean may intervene. Many of these species reappear in the mountains of southern Chili, Australia, Tasmania, and New Zealand. The southern temperate floras, on the other hand, of South America, South Africa, Australia, and New Zealand differ more among themselves than do the floras of Europe, northern Asia, and North America. To explain these facts Hooker suggested the probable former existence, during a warmer period than the present, of a center of creation of new species in the Southern Ocean, in the form of either a continent or an archipelago, from which the Antarctic flora radiated. From the zoological side a similar difficulty arises, and the hypothesis of a land-connection has been widely upheld, and that it existed as late as Mid-Tertiary times. The theory took a more definite form in the hands of Osborn (1900), who pictured relatively narrow strips of land connecting respectively South America on the one side and Tasmania and New Zealand on the other with the existing Antarctic land-area. This would accord well enough with the suggestion of Lothian Green, that the

plan of land-elevations on the earth is approximately tetrahedral; and it is, I believe, in line with the views of those who are best informed on antarctic geography and geology, as studied from the land itself. It may be hoped that further antarctic discovery may bring fresh facts to bear upon this question, for it is to the positive data acquired from study of the earth's crust that we must look, rather than to the exigencies of botanists and zoologists, for its final solution.

But the hypothesis of an Antarctic land-connection has been held open to doubt in various quarters. As Sir Wm. Thiselton Dyer has recently pointed out, Darwin himself dissented, though regretfully, from the sinking of imaginary continents in a quite reckless manner, and from the construction of land-bridges in every convenient direction. From the geological side Dana laid down the positive proposition that the continents and oceans had their general outline and form defined in earliest time. Sir John Murray, whose recent death we so deeply deplore, was an undeniable authority as to the ocean-floor. He wrote quite recently with regard to Gondwana-land, that "the study of ocean-depths and ocean-deposits does not seem in any way to support the view that continental land has disappeared beneath the floor of the ocean in the manner indicated." He suggested that the present distribution of organisms is better interpreted by the North Polar theory of origin. The "continuous current of vegetation" southward at the present time was recognized by Hooker himself, and definite streams of northern forms have been traced by him extending even to Australia and Tasmania. This might account for much in present-day distribution; though it seems doubtful whether it would fully explain the extraordinary distribution of Antarctic plants. The problem must for the present remain an open one.

This whole question, however, has a connection with the still wider difficulty of the existence within the polar area of ancient floras. In the north the fossils are even of sub-tropical character. Coal has been found in lands with a five months' night. How did such plants fare if the seasonal conditions were at all like the present? To explain this it would be a physiological necessity to assume either an entirely different climatal condition in those regions from that of the present time; or, as has been suggested, some shifting or creeping of the earth's crust itself. These are, however, questions which we can not undertake to discuss with effect in the Botanical Section. We must not do more than recognize that an unsolved difficulty exists.

We pass now from Hooker's great work to the last of the classical series, viz., the "Flora Australiensis" of Bentham and Baron Ferdinand von Müller. It is embodied in seven volumes, and was completed in 1878. Bentham, while assenting in his "concluding preface" to the principles laid down by Hooker in the Tasmanian flora, recognized as the chief component part of the present flora of Australia the indigenous genera and species, originated or differentiated in Australia, which never spread far out of it. Secondly, an Indo-Australian flora showing an ancient connection between Australia and the lands lying to the north. It is represented especially in tropical and subtropical east Queensland. Then there is the mountain flora common to New Zealand, and extending generally to the southern extra-tropical and mountain regions, while other constituents are ubiquitous maritime plants, and those which have been introduced since the European colonization. But the most remarkable, as they are

the least easily explained, are some few plants identical with species from North and West America, and from the Mediterranean. They are stated to be chiefly annuals, or herbaceous or shrubby plants; free-seeders; while their seeds long retain the power of germination. This may perhaps give the clue to this curious conundrum of distribution.

It has been fortunate that the duty of working out this remarkable flora should have fallen into the hands of such masters as Robert Brown, Sir Joseph Hooker, and Bentham. The foundations were thus surely laid. The further progress of knowledge has been carried on by the late Baron Ferdinand von Müller, and it may be confidently left in the hands of others who are still with us. The completion of the task of observing and recording may still be far ahead. But I may be pardoned if I utter a word of anticipatory warning. There is at the present time a risk that the mere work of tabulating and defining the species in a given country may be regarded as the only duty of a government botanist; that, whenever this is completed, his occupation will be gone. Some such erroneous idea, together with a short-sighted economy, is the probable explanation of the fact that certain positions hitherto held by professional botanists have recently been converted into positions to be held by agriculturists. In the countries where this has happened (and I refer to no part of Australasia) the vegetation had been very adequately, though not yet exhaustively, worked, as regards the flowering plants and ferns. But who that knows anything about plants would imagine that the ascription to a genus or order, or the designation by a couple of Latin names with a brief specific description, exhausts what it is important to know about a species? In most cases it is after this has been done that the real importance of its study begins. Such possibilities as these do not appear to have been appreciated by those who advised or controlled these official changes. I have no desire to undervalue the agriculturist or the important work which he does. But he is engaged in the special application of various pure sciences, rather than in pure science itself. Advance in the prosperity of any country which has progressed beyond the initial stages of settlement follows on the advance of such knowledge as the devotee of pure science not only creates, but is also able to inculcate in his pupils. It is then imperative that, in any state which actively progresses, provision shall be made for the pursuit of pure as well as of applied science. In my view an essential mistake has been made in changing the character of the appointments in question from that of botanists to that of agriculturists. For the change marks the abandonment of pure science in favor of its specialized and local application.

The head of such an institution should always be a representative of pure science, thoroughly versed in the nascent developments of his subject. He could then delegate to specialists the work of following out into detail such various lines of special application as agriculture, acclimatization, plant-breeding, forestry or economics. Or, if the organization were a large one, as we may anticipate that it would become in the capital of a great state, separate institutes might develop to serve the several applied branches, while to a central institute, in touch with them all, might be reserved the duty of advancing the pure science from which all should draw assistance and inspiration.

It matters little how this principle works out in detail, if only the principle itself be accepted, viz., that pure science is the fount

from which the practical applications spring. Sydney, as the capital of a great state, has already laid her course, as regards botanical science, in accordance with it. Her botanic garden and the recently developed botanical department in the university (which, I understand, may find its home ultimately in the botanic garden) will serve as centers of study of the pure science of botany. This will readily find its application to agriculture, to forestry, to economics, and in various other lines present and future. I am convinced that it is in the best interest of any state that can possibly afford to do so to encourage and liberally endow the central establishment where the pure science of botany is pursued, and to continue that encouragement and endowment, even though results of immediate practical use do not appear to be flowing from it at any given moment. For in these matters it is impossible to forecast what will and what will not be eventually of practical use. And in any case as educational centers the purely botanical establishments will always retain their important function of supplying that exact instruction, without which none can pursue with full effect a calling in the applied branches.

We may now turn from generalities to certain special points of interest in your peculiar flora which happen to have engaged my personal attention. They center round a few rare and isolated plants belonging to the Pteridophyta, a division of the vegetable kingdom which there is every reason to believe to have appeared early in the history of evolution. But though the type may be an ancient one it does not follow that every representative of it preserves the pristine features intact. Throughout the ages members of these early families may themselves have progressed. And so among them to-day we may expect to find some which preserve the ancient characters more fully than others. The former have stood still, and may be found to compare with curious exactitude with fossils even of very early date. The latter have advanced, and though still belonging to the ancient family, are by their modifications become essentially modern representatives of it. For instance, the fern *Angiopteris* has a sorus which very exactly matches sori from the Paleozoic period, and it may accordingly be held to be a very ancient type of fern. On the other hand, the genera *Asplenium*, or *Polypodium*, include ferns of a type which has not been recognized from early fossil-bearing rocks, and they may be held to be essentially modern. But still all of them clearly belong to the family of the ferns.

In the Australian flora only three of the four divisions of the Pteridophyta are represented. For, curiously enough, there does not appear to be any species on your continent of the widely spread genus *Equisetum*, the only living genus of that great phylum of the Equisetales, which figured so largely in the Paleozoic period; and this notwithstanding that one species (*E. debile*) is present among the Polynesian Islands. But all the three other divisions of the Pteridophyta are included, and are represented in each case by plants which show peculiar and, probably for the most part, archaic characters. I propose to sketch before you very briefly the points of interest which the more notable of these archaic types present. Some justification may be found for my doing so because nearly all of them have been submitted to detailed study in my laboratory in Glasgow, and much of the work has been done upon material supplied to me by your own botanists. I take this opportunity of offering to them collectively my hearty thanks.

The tenure by Dr. Treub of the office of

director of the botanic gardens of Buitenzorg was rendered famous by his personal investigations, and chiefly by his classical researches on the Lycopods. These were followed up by other workers, and notably by Bruchmann; so that we now possess a reasonable basis for comparison of the different types of the family as regards the prothallus and embryology, as well as of the sporophyte plant; and all of these characters must be brought together as a basis for a sound conclusion as to their phyletic seriation. The most peculiar living Lycopods are certainly *Isoëtes* and *Phylloglossum,* both of which are found in Australia. The former need not be specially discussed here, as it is a practically world-wide genus. It must suffice to say that it is probably the nearest living thing to the fossils *Lepidodendron* and *Sigillaria,* and may be described as consisting of an abbreviated and partially differentiated *Lepidostrobus* seated upon a contracted stigmarian base.

But *Phylloglossum,* which is peculiar to the Australasian region, naturally claims special attention. The plant is well known to botanists as regards its external features, its annual storage tuber, its leafy shoot with protophylls and roots, and its simple shaft bearing the short strobilus of characteristic Lycopod type. But its prothallus has never been properly delineated, though it was verbally described by Dr. A. P. W. Thomas in 1901.[2] Perhaps the completed statement may have been reserved as a pleasant surprise for this meeting. But the description of thirteen years ago clearly shows its similarity to the type of *Lycopodium cernuum.* The sporophyte compares rather with *L. inundatum.* Both of these are species which, though probably not the most primitive of the genus, are far from being the most advanced. As all botanists know, the question of the position of *Phylloglossum* chiefly turns upon the view we take of the annual tuber and its protophylls. Treub, finding similar conditions in certain embryos of Lycopods, called it a "protocorm," and believed that he recognized in it an organ of archaic nature, which had played an important part in the early establishment of the sporophyte in the soil, physiologically independent of the prothallus. I must not trouble you here with the whole argument in regard to this view. Facts which profoundly affect the conclusion are those showing the inconstancy of occurrence of the organ. Mr. Holloway has recently described it as of unusual size in your native *L. laterale,* as it is also in *L. cernuum.* But it is virtually absent in those species which have a large intraprothallial foot, such as *L. clavatum,* as well as in the genus *Selaginella* and in *Isoëtes.* In *L. Selago,* which on other grounds appears to be primitive, there is no "protocorm." Such facts appear to me to indicate caution. They suggest that the "protocorm" is an opportunist local swelling of inconstant occurrence, which, though biologically important in some cases, is not really primitive.

If this is the comparative conclusion, then our view will be that *Phylloglossum* is a type of Lycopod which has assumed, perhaps relatively recently, a very practical mode of annual growth. Related, as it appears to be on other points, with the *L. inundatum* group of species, it has bettered their mode of life. *L. inundatum* dies off each year to the very tip of its shoot, so that only the bud remains to the following season. It is notable that Goebel has described long ago how the young adventitious buds of this species start with small "protocorms," quite like those of *Phylloglossum* itself, or like the embryo of *L. cernuum.* And so we may conclude that in *Phylloglossum* a tuberous development, containing a store to start the plant in the spring,

[2] *Proc. Roy. Soc.,* Vol. 69, p. 285.

has been added to what is already seen normally each year in *L. inundatum.* And this mode of life of *Phylloglossum* begins, as Thomas has shown, with its embryo. This appears to me to be a rational explanation of the "protocorm" of *Phylloglossum;* but it robs the plant of much of its theoretical interest as an archaic form.

The phylum of the Sphenophyllales was originally based on certain slender straggling plants of the genus *Sphenophyllum* found in the Paleozoic rocks; but apparently died out in the Permian period. Your native genera *Tmesipteris* and *Psilatum* were ranked by earlier botanists with the Lycopods, but a better acquaintance with their details, and especially the examination of numerous specimens on the spot, indicated a nearer affinity for them with the Sphenophyllales. It was Professor Thomas who, in 1902, first suggested that the Psilotaceæ might be included with the Sphenophyllæ in the phylum of the Sphenophyllales, and I personally agree with him. Dr. Scott, however, dissents, on the ground that the leaves are persistently whorled in the sphenophylls, while they are alternate in the Psilotaceæ; and while the former branch monopodially the latter dichotomize. But since both of these characters are seen to be variable within the not far distant genus *Lycopodium,* the differences do not seem to me to be a sufficient ground for keeping them apart as the separate phyla of Sphenophyllales and Psilotales. Whatever degree of actual relation we trace, such plants as *Tmesipteris* and *Psilotum* are certainly the nearest living representatives of the Sphenophylleæ, a fact which gives them a special distinction. The Psilotaceæ also stand alone in the fact that they are the only family of the Pteridophytes in which the gametophyte is still unknown. They produce spores freely, but there the story stops. Any young Australian who hits upon the way to induce these recalcitrant spores to germinate, and to produce prothalli and embryos, or who found their prothalli and embryos in the open, would have before him a piece of work as sensational as anything that could be suggested. Further, I am told that *Tmesipteris* grows here on the matted stumps of *Todea barbara.* I shall be alluding shortly to the fossil Osmundaceæ. May we not venture to fancy the possibility of some fossil *Osmunda* being found which has embalmed for us among its roots a Mesozoic or even a Tertiary Sphenophyll? And thus a link might be found between the Paleozoic types and the modern Psilotaceæ, not only in time, but even in character.

We pass now to the last phylum of the Pteridophyta, the Filicales. I am bound to say that for me its interest far outweighs that of others, and for this reason: that it is represented by far the largest number of genera and species at the present day, while there is a sufficiently continuous and rich succession of fossil forms to serve as an efficient check upon our comparative conclusions.

Since 1890 it has been generally accepted that the Eusporangiate ferns (those with more bulky sporangia) were phyletically the more primitive types, and the Leptosporangiate (those with more delicate sporangia) the derivative, and in point of time later. The fossil evidence clearly upholds this conclusion. But, further, it has been shown that the character of the sporangium is merely an indicator of the general constitution of the plants in question. Where it is large and complex, as in the Eusporangiates, all the apical segmentations are, as a rule, complex, and the construction of the whole plant relatively bulky. Where the sporangium is delicate and relatively simple all the apical segmentations follow suit, and the construction of the

plant is on a less bulky model. On this basis we may range the ferns roughly as a sequence, starting from relatively bulky types of the distant past, and progressing to the more delicate types of the present day. The large majority of the living species belong naturally to the latter. But the former are still represented by a few genera and species which, like other survivals from a distant past, are frequently of very restricted distribution.

An interesting feature of the Australasian flora is that a considerable number of these relatively ancient forms are included in it. Thus the Marattiaceæ are represented by one species of *Marattia* and one of *Angiopteris*. Though in themselves interesting, they will be passed over without special remark, as they are very widely spread tropical forms.

All the three genera of Ophioglossaceæ are included, there being two species of *Ophioglossum* and two of *Botrychium*, while *Helminthostachys* is recorded from Rockingham Bay. This family is coming more than ever to the front in our comparisons, owing to their similarity in various aspects to the ancient Botryopterideæ. Though the Ophioglossaceæ have no secure or consecutive fossil history, still they may now be accepted as being very primitive but curiously specialized ferns. Perhaps the most interesting point recently detected in them is the suspensor found by Dr. Lyon in *Botrychium obliquum*, and by Dr. Lang in *Helminthostachys*. This provides a point for their comparison with the similar embryonic condition in *Danæa*, as demonstrated by Professor Campbell. The existence of a filamentous initial stage of the embryo is thus shown for three of the most primitive of living ferns. Its existence in all of the Bryophytes, and in most of the Lycopods, as well as in the seed-plants, is a very significant fact. Dr. Lang suggests that "the suspensor represents the last trace of the filamentous juvenile stage in the development of the plant, and may have persisted in the seed-plants from their filicineous ancestry." Such a possibility would fit singularly well with the theory of encapsulation of the sporophyte in the venter of the archegonium.

The representation of the ancient family of the Osmundaceæ in the Australasian flora is very fine, though limited to five living species, while *Osmunda* itself is absent. It is, however, interesting that the family dates back locally to early fossil times. It was upon two specimens of *Osmundites* from the Jurassic rocks in the Otago district of New Zealand that the series of remarkable papers on "The Fossil Osmundaceæ" by Kidston and Gwynne-Vaughan was initiated. It is no exaggeration to say that these papers have done more than any other recent researches to promote a true understanding not only of the Osmundaceæ themselves, but of fern-anatomy as a whole. They have placed the stellar theory in ferns for the first time upon a basis of comparison, checked by reference to stratigraphical sequence. It would be leading us too far for me to attempt here to summarize the important results which have sprung from the study of those fossils, so generously placed by Mr. Dunlop in the hands of those exceptionally able to turn them to account. It must suffice to say that it is now possible to trace as a fairly continuous story the steps leading from the protostelic state to the complex condition of the modern *Osmunda*. These facts and conclusions are to be put in relation with the anatomical data fast accumulating from the Ophioglossaceæ in the hands of Professor Lang and others. From such comparisons a rational explanation of the evolutionary steps leading to the complex stellar state in ferns at large begins to emerge. This is no mere tissue of

surmise, for the conclusions are based on detailed comparison of types occurring in lower horizons with those of the present day.

I must pass over with merely nominal mention your interesting representation of the ancient families of Schizæaceæ, Gleicheniaceæ and Hymenophyllaceæ, all of which touch the very foundations of any phyletic system of ferns. Also the magnificent array of Dicksonieæ and Cyatheæ, and of the important genus *Lindsaya*—ferns which take a rather higher position in point of view of descent. But I am bound to devote a few moments to one of your most remarkable ferns, endemic in New Zealand—the monotypic *Loxsoma.*

This species has peculiar characters which justify its being regarded systematically as the sole representative of a distinct tribe. It is also restricted geographically to the North Island of New Zealand. These facts at once suggest that it is an ancient survival, a conclusion with which its solenostelic axis, its sorus and sporangium, and its prothallus readily accord. I have lately shown that the Leptosporangiate ferns fall into two distinct series, those in which the origin of the sorus is constantly superficial, and those in which it is as constantly marginal. *Loxsoma* is one of the "Marginales." It shares this position with the Schizæaceæ, Thyrsopterideæ, Hymenophyllaceæ and Dicksonieæ, and the derivatives Davalieæ and Oleandreæ. Its nearest living relative is probably *Thyrsopteris,* which is again a monotypic species endemic in the island of Juan Fernandez. There is also a probable relation to the genus *Loxsomopsis,* represented by one species from Costa Rica, and a second lately discovered in Bolivia. Such a wide and isolated distribution of types, which by their characters are certainly archaic, suggests that we see in them the relics of a Filicineous state once widely spread, which probably sprang from a Schizæaceous source, and with them represent the forerunners of the whole marginal series. If we look for further enlightenment from the fossils, it is to the secondary rocks that we should turn. It is then specially interesting that Mr. Hamshaw Thomas has lately described a new Jurassic fern, *Stachypteris Halli,* which has marginal sori, and is probably referable to a position like that of *Loxsoma* and *Thyrsopteris,* between the Schizæaceæ and the Dicksonieæ. In fact the gaps in the evolutionary series of the Marginales are filling up. We may await with confidence fresh evidence from the Jurassic period, upon which Professor Seward is directing an intensive interest.

I should be ungrateful indeed if I did not mention your very full representation of Blechnoid ferns: for developmental material of several of these has been sent to me by Dr. Cockayne, and others from New Zealand. A wide comparative study of the genus has led me to somewhat unexpected results in regard to the plasticity of the sorus, its phyletic fusions and disruptions. The consequent derivative forms are seen in *Woodwardia* and *Doodya,* on the one hand, and on the other in *Scolopendrium* and *Asplenium.* These ferns together constitute a coherent phylum springing ultimately from a Cyatheoid source. The details upon which this conclusion is based I hope to describe in a separate communication to the section.

And lastly, the Hydropterideæ deserve brief mention. Represented in your flora by two species of *Azolla,* and one each of *Marsilea* and *Pilularia,* they typify a condition which must theoretically have existed among ferns in very early times, viz., the heterosporous state. But hitherto, notwithstanding the existence of our living Hydropterideæ, no fossil fern with microscopic

structure preserved had been detected from the primary rocks, showing this intermediate condition between the homosporous type and that of the Pteridosperms. This unsatisfactory position has now been resolved by Professor Lignier, who has recently described, under the name of *Mittagia,* a fossil from the Lower Westphalian, which bore sori of which the sporangia contained four megaspores, while the outer tissues of the sporangia resembled those of *Lagenostoma.* Pending the discovery of further specimens, these observations may be welcomed as filling with all probability a conspicuous gap in the evolutionary sequence of known forms.

From the rapid survey which I have been able to give you of some of the more notable Australasian ferns of relatively archaic type, it is clear that they have a very interesting and direct bearing upon the phylesis of ferns. The basis upon which conclusions as to phyletic sequence are arrived at is at root that of the natural system of classification generally—the recognition not of one character, or of two, but of as many as possible, which shall collectively serve as criteria of comparison. In the case of the Filicales we may use the characters of:—

(i) External form.
(ii) Constitution, as shown by simple or complex segmentation.
(iii) Dermal appendages, hairs or scales.
(iv) Stellar structure, simple or complex.
(v) Leaf-trace, coherent or divided.
(vi) Soral position.
(vii) Soral construction.
(viii) Indusial protections.
(ix) Sporangial structure and mechanism of dehiscence.
(x) Spore-output.
(xi) Spore-form and character of wall.
(xii) Form of prothallus.
(xiii) Position of the sexual organs, sunken or superficial.
(xiv) Number of spermatocytes and and method of dehiscence.
(xv) Embryology.

In respect of all these criteria progressions of character may be traced as illustrated by known ferns, and probably other criteria may emerge as study progresses. In each case, upon a footing of general comparison, checked as opportunity offers by reference to the stratigraphical sequence of the fossils, it may be possible to distinguish with some degree of certainty what is relatively primitive from what is relatively advanced. Thus, the protostele is generally admitted to be more primitive than the dictyostele, the simple hair than the flattened scale, and a high spore-output than a low one.

Applying the conclusions thus arrived at in respect to the several criteria, it becomes possible upon the sum of them to lay out the species and genera of ferns themselves in series, from the primitive to the advanced. In proportion as the progressions on the basis of the several criteria run parallel, we derive increased assurance of the rectitude of the phyletic sequences thus traced, which may finally be clinched, as opportunity offers, by reference to the stratigraphical occurrence of the corresponding fossils. This is in brief the phyletic method, as it may be applied to ferns. It may with suitable variation be applied to any large group of organisms, though it is seldom that the opportunities for such observation and argument are in any sense commensurate with the requirements. Perhaps there is no group of plants in which the opportunities are at the moment so great as in the Filicales, and they are yielding highly probable results from its application.

The greatest obstacle to success is found

in the prevalence of parallel development in phyla which are believed to have been of distinct origin. This is exemplified very freely in the ferns, and the systematist has frequently been taken in by the resemblances which result from it. He has grouped the plants which show certain common characters together as members of a single genus. Sir William Hooker in doing this merged many genera of earlier writers. His avowed object was not so much to secure natural affinity in his system as readiness of identification: and consequently in the "Synopsis Filicum" there are nominal genera which are not genera in the phyletic sense at all. For instance, *Polypodium* and *Acrostichum,* as there defined, may be held from a phyletic point of view to be collective groupings of all such ferns as have attained a certain state of development of their sorus; and that they are not true genera in the sense of being associated by any kinship of descent: this is shown by the collective characters of the plants as a whole. Already at least four different phyletic sources of the Acrostichoid condition have been recognized, and probably the sources of the Polypodioid condition are no fewer. Such "genera" represent the results of a phyletic drift, which may have affected similarly a plurality of lines of descent. It will be the province of the systematist who aims at a true grouping according to descent to comb out these aggregations of species into their true relationships. This is to be done by the use of wider, and it may be quite new criteria of comparison. Advances are being made in this direction, but we are only as yet at the beginning of the construction of a true phyletic grouping of the Filicales. The more primitive lines are becoming clearer: but the difficulty will be greatest with the distal branches of the tree. For these represent essentially the modern forms, they comprise the largest number of apparently similar species, and in them parallel development has been most prevalent.

If this difficulty be found in such a group as the Filicales, in which the earlier steps are so clearly indicated by the related fossils, what are we to say for the Angiosperms? Our knowledge of their fossil progenitors is very fragmentary. But they are represented now by a multitude of forms, showing in most of their features an irritating sameness. For instance, vascular anatomy, that great resource of phyletic study in the more primitive types, has sunk in the Angiosperms to something like a dead level of uniformity. There is little variety found in the contents of embryo-sacs, in the details of fertilization, or in embryology. Even the ontogeny as shown in the seedling stages affords little consolation to the seeker after recapitulation. On the other hand, within what are clearly natural circles of affinity there is evidence of an extraordinary readiness of adaptability in form and structure. Such conditions suggest that we see on the one hand the far-reaching results of parallel development, and on the other the effects of great plasticity at the present day, or in relatively recent times. Both of these are points which prevent the ready tracing of phyletic lines. In the absence of reliable suggestions from paleontology, the natural consequence is the current state of uncertainty as to the phyletic relations of the Angiosperms.

Various attempts have been or are being made to meet the difficulty. Some, on the basis of the recent observations of Wieland and others, are attempting along more or less definite monophyletic lines to construct, rather by forcible deduction than by any scientific method of induction, an

evolutionary story of the Angiosperms. I do not anticipate that any great measure of success, beyond what is shown in a very polysyllabic terminology, and an appearance of knowing more than the facts can quite justify, will attend such efforts. It would seem to me to be more in accord with the dictates of true science to proceed in a different way, as indeed many workers have already been doing. To start not from preconceptions based upon limited paleontological data, but from an intensive study of the living plants themselves. To widen as far as possible the criteria of comparison, by making, for instance, every possible use of cellular, physiologico-chemical, and especially secretory detail, and of minor formal features, such as the dermal appendages, or by initiating a new developmental morphology of the flower from the point of view of its function as a whole; and with its physiological end clearly in sight, viz., the maturing, nourishing, and placing of new germs. To make on some such basis intraordinal, and intrageneric comparisons with a view to the phyletic seriation of closely related forms; and so to construct probable short series, which may subsequently be associated into larger phyletic groupings. This should be checked wherever possible by physiological probability. A keen eye should be kept upon such information as geographical distribution and paleontology may afford, and especially upon the fossils of the Mesozoic Period. What is above all needed for success among the Angiosperms is new criteria of comparison, to meet the far-reaching difficulties that follow from parallel development and recent adaptation. If some such methods be adopted, and strenuously pressed forward, the task should not appear hopeless, though it can not be anything else than an arduous one.

I can not conclude without some remark on the bearing of parallel or convergent development, so fully exemplified in the Filicales, upon the question of the genesis of new forms. Any one who examines, from the point of view suggested in this address, the larger and well-represented divisions of the vegetable kingdom must be impressed with the extraordinary dead level of type to which their representatives have attained. In most of these divisions the phyletic history is obscured, partly by the absence of any consecutive paleontological record, but chiefly by the want of recognized criteria for their comparison. This is very prominently the case for the mosses, and the Angiosperms.

But it may be doubted whether these large groups differ in any essential point, in respect of the genesis of their multitudinous similar forms, from the Filicales, in which the lines of descent are becoming clearer through additional knowledge. Suppose that we knew of no fossil ferns; and that none of the early fern-types included under the term "Simplices" had survived in our living flora: and that the Filicales of our study consisted only of the 2,500 living species of the old undivided genera of *Polypodium, Asplenium, Aspidium* and *Acrostichum.* Then the phyletic problem of the Filicales would appear as obscure as does that of the mosses, or of the Angiosperms of the present day. They would present, as these great groups now do, an apparent dead level of sameness in type, though the phyletic starting-points in each may have been several and distinct. There is every reason to suppose that in the phylesis of the mosses or the Angiosperms also there has been a parallel, and even a convergent, development of the same nature as that which can be cogently traced in the Filicales: but that it is obscured by the obliteration of the early stages. Internal evidence from their com-

parative study fully justifies this conclusion. How, then, are we to regard this insistent problem of parallelism and convergence from the point of view of genetic study?

A belief in the "inheritance of acquired characters," or, as it is sometimes expressed, "somatic inheritance," is at present out of fashion in some quarters. But though powerful voices may seem to have forced it for the moment into the background, I would take leave to point out that such inheritance has not been disproved. All that has been done, so far as I understand the position, is to show that the evidence hitherto advanced in support of it is insufficient for a positive demonstration. That is a very different thing from proving the negative. We hear of "fluctuating variations" as distinct from "mutations"; and it is asserted that the former are somatic, and are not inherited, while the latter are inherited. This may be held as a useful terminological distinction, in so far as it accentuates a difference in the heritable quality. But it leaves the question of the origin of these heritable "mutations" quite open. At the present moment I believe that actual knowledge on this point is very like a complete blank. Further, it leaves indefinite the relative extent and proportion of the "mutations." It is commonly held that mutations are considerble deviations from type. I am not aware that there is any sufficient ground for such a view. It may probably have originated from the fact that the largest are most readily observed and recognized as reappearing in the offspring. But this is no justification for ignoring the possibility of all grades of size or importance of heritable deviations from type.

On the other hand, adaptation, with its consequence of parallel or even convergent development in distinct stocks, is an insistent problem. The real question is, What causes are at work to produce such results? They are usually set down to the selection of favorable divergences from type out of those produced at random. But the prevalence of parallelism and convergence suggests that those inheritable variations, which are now styled "mutations," are not produced at random. The facts enforce the question whether or not they are promoted and actually determined in their direction, or their number, or their quality, in some way, by the external conditions. Parallelism and convergence in phyletic lines which are certainly distinct impress the probability that they are. Until the contrary is proved it would, in my opinion, be wiser to entertain some such view as a working hypothesis than positively to deny it. Such a working hypothesis as this is not exactly the same as a "mnemic theory," though it is closely akin to it. It may perhaps be regarded as the morphologist's presentation, while the mnemic theory is rather that of the physiologist. But the underlying idea is the same, viz., that the impress of external circumstance can not properly be ruled out in the genesis of inheritable characters, simply because up to the present date no definite case of inheritance of observable characters acquired in the individual lifetime has been demonstrated. Of course, I am aware that to many this is flat heresy. At this meeting of the association it amounts almost to high treason. I plead guilty to this heresy, which may by any sudden turn of observation be transformed into the true faith. I share it in whole or in part with many botanists, with men who have lived their lives in the atmosphere of experiment and observation found in large botanical gardens, and not least with a former president of the British Association—viz., Sir Francis Darwin.

It is noteworthy how large a number of botanists dissent from any absolute negation of the influence of the environment upon the genesis of heritable characters. Partly this may be due to a sense of the want of cogency of the argument that the insufficiency of the positive evidence hitherto adduced justifies the full negative statement. But I think it finds its real origin in the fact that in plants the generative cells are not segregated early from the somatic. In this respect they differ widely from that early segregation of germ-cells in the animal body, to which Weismann attached so much importance. The fact is that the constitution of the higher plants and of the higher animals is in this, as in many other points, radically different, and arguments from the one to the other are dangerous in the extreme. Those who interest themselves in evolutionary questions do not, I think, sufficiently realize that the utmost that can be claimed is analogy between the higher terms of the two kingdoms. Their phyletic separation certainly dates from a period prior to that of which we have any knowledge from the fossil record. Let us give full weight to this fact, as important as it is indisputable. The early definition of germ-cells in the animal body will then count for nothing in the evolutionary problem of plants. Moreover, we shall realize that the plant, with its late segregation of germ-cells, will present the better field for the inquiry whether, and how far, the environment may influence or induce divergences from type. From this point of view the widespread opinion among botanists that the environment in some sense determines the origin and nature of divergences from type in plants should command a special interest and attention.

I must now draw to a close. I have passed in review some of your more notable plants, and pointed out how the Australasian flora, whether living or fossil, includes in unusual richness those evidences upon which the fabric of evolutionary history is being based. I have indicated how this history in certain groups is showing ever more and more evidence of parallel development, and that such development, or convergence, presses upon us the inquiry into the methods of evolutionary progress. The illustrations I have brought forward in this address clearly show how important is the positive knowledge derived from the fossils in checking or confirming our decisions. Paleophytology is to be prized not as a separate science, as, with an enthusiastic view restricted between blinkers, a recent writer has endeavored to enforce. To treat it so would be to degrade it into a mere side alley of study, instead of holding it to be the most positive line that we possess in the broad avenue of botanical phylesis. An appreciation of such direct historical evidence is no new idea. Something of the same sort was felt by Shakespeare three centuries ago, and it remains the same to-day. Nay more:—it may lead us even to forecast future possibilities. In following our evolutionary quest in this spirit we shall find that we are indeed—

> Figuring the nature of the times deceased,
> The which observed, a man may prophesy
> With a near aim, of the main chance of things
> As yet not come to life.
>
> (King Henry IV., Part II., Act iii, Scene i.)

F. O. Bower

THE DECREASING BIRTH RATE OF THE GERMAN EMPIRE

During the 30 years following the war with France the population of Germany increased enormously while the population of France remained almost stationary. But at the beginning of the new century the birth rate in Germany began to decline and is still declining at a rapid rate. In an article in No. 18 of